LETTERS TO THE EDITOR.

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Recent Exploration in the Upper Air and its Bearing on the Theory of Cyclones.

ABOUT ten years ago there was an interesting discussion in regard to the theory of cyclones, by such leaders in meteorology as Ferrel, Blanford, Hann and Davis (see NATURE, vol. xliii. p. 82 and p. 470). Since then, considerable new material has been accumulated by research in the free air with kites and balloons, and it seems appropriate to consider its bearing on current theories.

In America the work with kites, by Mr. Rotch, has resulted in the discovery of the following facts :—

(1) The atmosphere is separated by sharply marked inverted vertical gradients of temperature into superposed strata, each stratum potentially warmer than the one beneath. By potentially warmer is meant that if any stratum were brought down, it would be heated by compression and become warmer than the stratum it replaced. There are usually two, and sometimes three, strata between the ground and the altitude of 3000 metres. The boundaries between these strata are regions of sharp contrast in a vertical direction of temperature, of humidity (both absolute and relative), and sometimes of wind direction. These boundaries are also regions of maxima in wind velocity, and the regions where clouds are chiefly found.

(2) In the changes of condition of the atmosphere from day to day, the minima of temperature and humidity occur simultaneously at all levels, except that in a superficial stratum within about 300 metres of the ground, the minimum sometimes occurs later as a result of surface cooling.

(3) The air column up to 3000 metres above barometric minima at sea-level averages about 10° F. warmer than the air column up to 3000 metres above barometric maxima at sea-level.

(4) All the conditions which characterise the surface cyclone and anticyclone, such as the circulation of the wind around a central area, the clouds and the rainfall, usually do not exceed the height of 3000 metres. Above that height there is an entirely different distribution of pressure and wind circulation from that observed at the earth's surface.

(5) In the areas of low pressure in the upper atmosphere the air is cold, extremely dry and clear. In the areas of high pressure in the upper atmosphere the air is warm and frequently moist.

In kite-flights made on November 24 and 25, 1898, at Blue Hill, there were evidences of three distinct wind circulations. The surface cyclone did not exceed 800 metres (or half a mile) in thickness, and above this was a warm-centre cyclone with dense clouds and precipitation about 2000 metres in thickness. At the height of 3000 metres (or about two miles) the wind, on November 24, was found blowing from the south and circulating around an area of low pressure with a cold, dry central area; while at the same time at the surface of the earth the wind was from the north and circulating around a warm-centre surface cyclone. (See *Bulletins* No. 1, 1899, and No. 1, 1900, of the Blue Hill Meteorological Observatory).

In France, M. Teisserenc de Bort has made a study of the air by means of *ballons sondes* launched at frequent intervals from Trappes. His results show that the annual period in the temperature of the air is well marked up to and exceeding ten kilometres. They show further that during the irregular warm and cold periods in the atmosphere the isotherms rise and fall simultaneously at all heights up to at least ten kilometres. In other words, the warm and cold waves aloft occur simultaneously with those near the surface (see *Comptes rendus*, August 21, 1899).

Dr. Hergesell, of Strassburg, has discussed the records of the international balloon ascents, and derived a number of important conclusions. Among these are: (1) In the highest strata of the atmosphere attainable by balloons the temperature change from day to day, and the temperature gradients in a horizontal direction, are very marked. Within distances of only a few hundred kilometres are sometimes temperatures at the same level which differ as much from each other as 30°–40° C. (2) Such temperature distributions as that which brought frosts in Europe on May 13, 1897, are not local and confined to the earth's surface, but meteorological phenomena of great extent and importance which embrace the entire atmosphere above Europe. (3) By computing and plotting the air-pressure for the heights of 5000 and 10,000 metres, it was found that the areas of low pressure at these heights coincide approximately with the areas of low temperature, and in most cases are many hundreds of kilometres from the surface cyclone. Thus, on March 24, 1899, the surface cyclone or area of low pressure was along the north coast of the Mediterranean, near Italy, while the minimum pressure at 5000 and 10,000 metres was in Finland, or even further north (*Meteorologische Zeitschrift*, January 1900).

To compare these facts with theory, I have looked up the views expressed in modern text-books and recent literature. I find a number of different opinions in regard to the causes of cyclones, and have classified them as follows :—

(1) Instability produced by a rapid decrease of temperature with increase of height; that is, by a vertical gradient equalling or exceeding the adiabatic rate. This may be called *the theory of vertical instability*.

(2) Instability produced by differences of temperature in a horizontal direction. In such a case, in consequence of the the difference in density, there is established a convectional interchange of air between areas of different temperatures, and there result differences of pressure, and consequently cyclones and anticyclones. This theory I call *the convection theory*.

(3) If a current of damp air is deflected upward by any means, mechanical or otherwise, it cools by expansion, and its moisture begins to condense. This condensation retards further cooling, so that the air in rising may cool at a rate less rapid than that ordinarily existing in the atmosphere. In such a case, the air would continue to rise, and the conditions would be favourable for storm formation, as long as the supply of moisture lasted. This has been called *the condensation theory*.

(4) When bodies of water, moving in different directions or with different velocities in the same direction, come in contact, whirls and eddies are set up between them. It is therefore conceivable that the large masses of air moving between the equator and the pole, may, at their places of meeting, produce similar large eddies, such as the cyclones of the weather map. These have been called *dynamic cyclones* and also *driven cyclones*.

Probably no meteorologist believes that any one of these causes acts entirely alone in cyclone formation. But a difference of opinion arises as to which is the principal cause, and to what extent the others are subsidiary causes. All theories agree in ascribing the primary cause to differences of temperature, either local or between equator and pole.

Vertical instability can scarcely be considered the primary cause of cyclones, because, as stated above, the atmosphere is found normally separated into strata, each one potentially warmer than the one beneath. The fatal objection to the condensation theory, as pointed out by Dr. Hann, is that cyclones in temperate latitudes are more violent in winter than in summer. Latent heat is not so effective an aid to storm action in winter as in summer, and yet it is in winter that our cyclones possess their greatest violence. According to the theory of driven cyclones, "The masses of air set in motion polewards by the upper gradients are resolved, in part, into great whirls, the principal progressive motion of which is controlled by the prevailing west component of the former. The influence of the inequalities of the earth's surface, the different heating and cooling of the land and ocean, and the bringing in of aqueous vapour and its condensation, come thus into account, as matters of secondary importance," (NATURE, vol xliii. p. 470). "If . . . cyclonic and anticyclonic disturbances are produced by the irregular flow of the general winds, it is probable that these disturbances would originate in the higher regions of the atmosphere, where the winds blow much faster than near the earth's surface. The differences of pressure produced at high altitudes would be felt down to sea-level; and, as the lower winds move with comparative slowness, they would be governed by the gradients thus imposed on them by the irregular movements of the upper winds. According to this theory, an area of high pressure would be perceived at sea-level beneath a district where the upper currents crowd together; and an area of low pressure, or a cyclonic storm, would be developed beneath a region where the upper currents are somewhat divergent." (Davis's "Elementary Meteorology," 1894, p. 219.)

As stated above, observation does not show areas of minimum pressure in the upper air above areas of minimum pressure at sea-level. Furthermore, according to the theory of driven cyclones, the progressive motion of the cyclone is supposed to be determined by the "prevailing west component" of the upper currents. At Blue Hill the mean westerly component of the upper current is 35 metres per second at 9000 metres, about 17 metres per second at 4000 metres, and 1 metre per second at 200 metres (*Harvard Observatory Annals*, 1890, vol. xl. p. 447). It is natural to suppose that a driven whirl, in such conditions, would be rapidly toppled over and destroyed. Yet storms persist for days. If, however, a driven whirl did persist in such conditions, its axis if tilted at all would, according to all analogy, be tilted in the direction of progressive motion. Yet the direct observations with kites at Blue Hill, and the observations of clouds by Ley in England, prove that the axis of the cyclone is tilted backward. Moreover, it is reasonable to suppose that the air in the rear of a driven whirl would partake of the progressive motion of the whirl, and this, added to the indraught, would make the wind velocity in the rear of the whirl very much greater than that of the winds in front, yet such is not generally found to be the case. For these reasons I think the observations do not favour the theory of driven cyclones.

The theory of cyclones with which the observations in temperate latitudes seem best to agree, is the theory which supposes the cyclone to result from contrast of temperature in a horizontal direction. This I have called the *convection theory*. In this theory there are two types of cyclone. The warm-centre cyclone of the lower atmosphere, and the cold-centre cyclone of the upper atmosphere. The best type of the cold-centre cyclone is the polar cyclone; but there also exist in the upper air in temperate latitudes small travelling cyclones or *hemi*-cyclones of the same nature. Horizontal contrasts of temperature are most marked in winter, hence the theory explains why cyclones are most violent in winter. The origin of the horizontal contrasts of temperature is not shown by observation. They probably arise by the interchange of air between higher and lower latitudes. A body of air moving from the equator toward high latitudes would come into a region where it would be nearly surrounded by colder air, and the conditions would favour the production of a warm-centre cyclone. A body of air moving toward the equator would produce conditions favourable to a cold-centre cyclone. H. H. CLAYTON.

Blue Hill Meteorological Observatory, March 30.

Rock-structures in the Isle of Man and in South Tyrol.

IF the intercrossing of two separate systems of folding be the essential condition in the complicated rock-structures so ably worked out by Mrs. M. M. Ogilvie Gordon in South Tyrol, I scarcely think the parallelism with the conditions in the Manx *Carboniferous* rocks can be so close as Mrs. Gordon suggests in her recent letter (NATURE, March 22, p. 490).

So far as I have been able to judge, the disturbances in the Carboniferous volcanic rocks of the Isle of Man were the result of a movement which was single both as regards direction and time. It is true that this conclusion was reached in 1897, before Mrs. Gordon had taught us the importance of torsion-structure in areas of disturbance; but I re-examined the sections last autumn, after having studied Mrs. Gordon's paper, without finding any reason to alter my former opinion on this point. The interpretation given in my recent paper (*Q.J.G.S.* vol. lvi. p. 11) is therefore in all respects the same as that published in brief in the official Summary of Progress of the Geological Survey for 1897 (pp. 110-112).

It seems necessary, also, to call attention to the small scale of the structures in question in the Manx Carboniferous rocks. Their most striking feature is their sudden local development in a limited tract where the strata are rendered by diverse lithological composition peculiarly susceptible to differential displacement. Under such conditions, it appears that even a small degree of lateral movement may be so focussed as to cause great disturbance at certain places without much disturbance of adjacent tracts. The post-Carboniferous movement in the Isle of Man can scarcely have been even approximately of the magnitude of the disturbances in South Tyrol described by Mrs. Gordon.

It was in pre-Carboniferous times that the Manx region underwent earth-movements of really grand intensity; and Mrs. Gordon may have had this fact in mind in referring to the subject. In the Older Palæozoic (probably Cambrian) slate-rocks of the island, "crush-conglomerate" has been developed on a very extensive scale by differential shearing, as described by Prof. W. W. Watts and myself five years ago (*Q.J.G.S.* vol. li. p. 563). These rocks, moreover, show evidence of successive epochs of disturbance, varying slightly in direction but apparently all pre Carboniferous. The production of the "crush-conglomerate" appears to have occurred during only one of these stages. It is not improbable that an observer acquainted with the "torsion-structures" of the Dolomites might be able to find parallel phenomena among the highly complicated pressure-structures in the Manx Slates; but I think that a sharp distinction should be drawn between these structures and those of the Carboniferous rocks of the island. G. W. LAMPLUGH.

Tonbridge, April 8.

Electric Light Wires and Dust.

I BELIEVE that the collection of dust upon electric light wires and fittings is generally attributed to air currents, due to thermal causes, the same thing occurring, to some extent, with hot-water pipes. Recent experience has, however, convinced me that in the case of electric light conductors, electrostatic attraction is really the chief factor, particularly where the supply is at 200 volts from the street mains. In my office here I have several electric light cords strung across the ceiling. They are all exactly similar and under the same conditions, except that some of them have the switch in the negative and some in the positive conductor. The former gather dust to an extraordinary degree, and now, after a few months' use, have become quite an eyesore. The latter are practically as clean as when first put up. As is well known, the negative conductor of a street supply tends always to earth itself, and, as a matter of fact, in my case I find that the negative of my supply from the Westminster Co. is almost at earth potential. The positive, on the other hand, is nearly 200 volts above the potential of the earth. In this lies obviously the cause of the phenomenon. The wires which have the switch in the negative are nearly at 200 volts potential above the earth whenever the switch is off, while those which have the switch in the positive are at zero potential in these circumstances. Of course, when the switches are on, all the cords are under similar conditions, one conductor in each being nearly at 200 volts above earth, and the other at about earth potential. No doubt it is when the switch is off, in the case where it is in the negative conductor, that the accumulation of dust takes place. Having regard to the comparative lowness of the 200 volts potential, from an electrostatic point of view, the rate at which the dust accumulates on the cords is most surprising, and this is my reason for thinking it worth while bringing the matter to notice. A. A. C. SWINTON.

66 Victoria Street, Westminster, April 23.

ON THE SIZE AT WHICH HEAT MOVEMENTS ARE MANIFESTED IN MATTER.

IN the molecular theory of heat it is assumed that the motions of atoms and molecules are the motions upon which the phenomena of temperature depend. These motions are assumed to be very irregular, and the apparent uniformity of structure of a gas, for example, is attributed to the very small size and irregularity of the motions, which within any region of sensible size are the same, *on the average*, as within any neighbouring region. Within regions of molecular dimensions the distribution of motion is extremely irregular; neighbouring molecules are not in general moving at the same speed or possibly vibrating in the same way. Hence in this view the size at which heat movements are manifested in matter are of molecular sizes, *i.e.* from 10^{-7} to 10^{-8} cm.

In addition, however, to all this matter, motion and vibration, there are present ether motions of an irregular kind. Within any closed envelope at constant temperature the ether motions must be in statistical equilibrium with the motions in the envelope. The energy per c.c. of these ether motions will be considerable at high temperatures, and small at low ones. Many years ago I called attention to the energy per c.c. required in order to, in this sense, warm up ether, and showed that it was quite comparable with that required to warm up a rare